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(Buijs's Ballot)

ROYAL DUTCH METEOROLOGICAL INSTITUTE.

SUGGESTIONS

ON A

UNIFORM SYSTEM

OF

METEOROLOGICAL OBSERVATIONS.

UTRECHT,
Printing Office „The Industry.”
(K. A. MANSSEN.)
1872.

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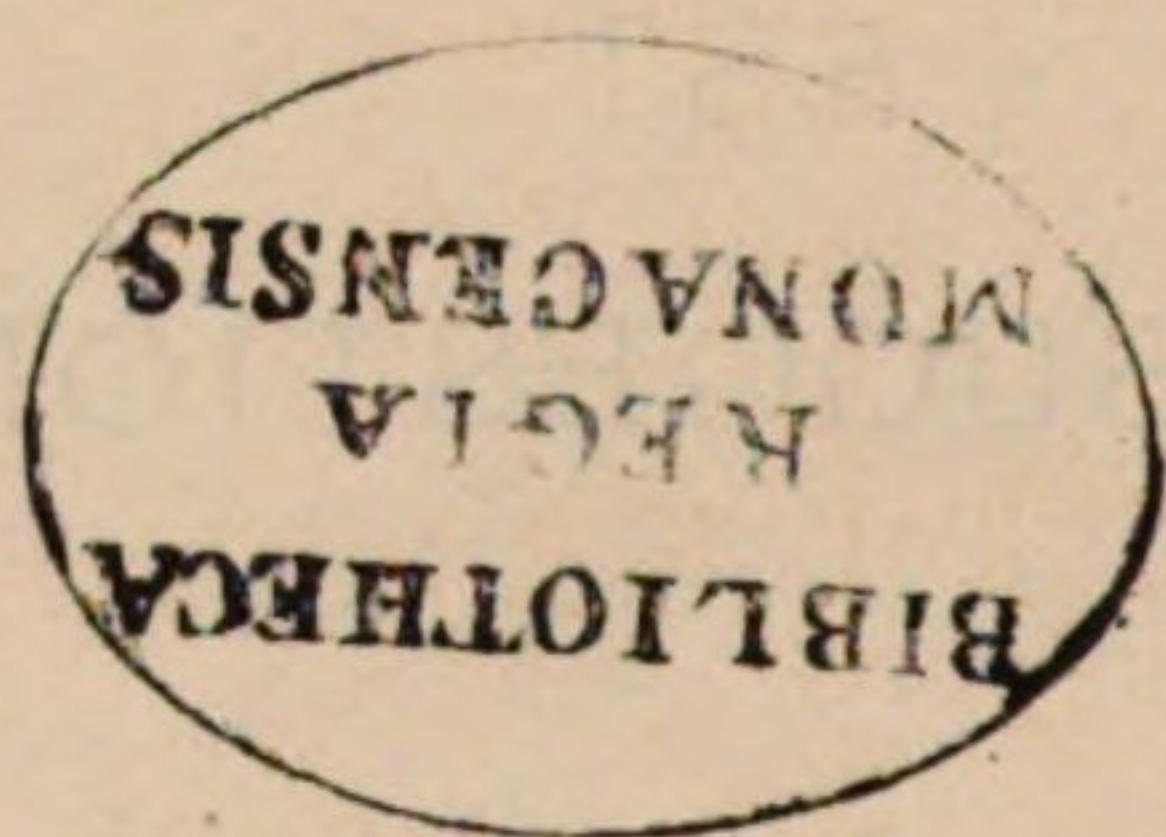
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## PREFACE.

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*In the following pages I have endeavoured to review some of the questions, that may be discussed at a meeting of the General Congress of Meteorologists to be held, probably at Vienna, in the course of the present Year.*

*The want of such a Congress is deeply felt, and, whenever it meet, it will be of great utility. Nor can it be but a saving of much time, if attention be previously directed, however imperfectly, to such subjects as are likely to be submitted to consideration.*

*In this paper I have adverted only incidentally to foreign inquiries, the great scientific value of which I am eager to acknowledge.*

*An apology is perhaps due for this apparent neglect.*

*M. Muhry's inquiry into the local influence of mountains, of rain and wind, and the investigations of the progress of storms by Messrs Buchan, Dove Clement Ley, Loomis, Moon, Le Verrier, and especially of the origin of depressionsystems, the great problem to be solved, have engaged my most serious*



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attention. I should have dwelt at some length on Mr. Clement Ley's work „The laws of the winds prevailing in Europe,” in which the writer agrees on many important points with Prof. Mohn, but unfortunately his publication did not reach me, until the following pages had been printed.

My principal aim, however, has been to draw attention to the general method of observing, and of printing the facts observed at the least expense and so as to facilitate particular investigations.

I entreat the reader's indulgence, if I appear to insist too much on such instructions as I have observed myself, or dwell too long on researches made by the Dutch Meteorological Institute. I should be sorry, if this were imputed to my not sufficiently appreciating the investigations and labours of others.

Zealously as I have advocated my opinion, I am most anxious to do ample justice to those distinguished philosophers, my fellow labourers, and shall pay due deference to their opinions by eagerly complying with such of their wishes as may be generally accepted by the Congress.

The Dutch language is seldom known beyond the confines of our own country, and this circumstance, joined to the imperfect character of our researches, has caused them to be taken little notice of.

Hence I have deemed it advisable to have recourse to another medium.

While in German more general questions have been discussed, the English language is perhaps more widely known, I have therefore selected it for this communication. This will sufficiently account for the many



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*imperfections in the style of this paper. Yet sensible as I am of never having attained that clearness and propriety of diction, so characteristic of English writers, I hope I have succeeded everywhere in making myself intelligible to those to whom these pages are more specially addressed.*

Utrecht, 10 Januarij 1872.

BUIJS BALLOT.









Soon after the establishment of the R. D. Meteorological Institute I expressed a hope that some meteorologists would assemble in Congress and deliberate on the interests of Meteorology, that they might draw up some rules to be followed in making observations, in directing their calculations, and agree on the form in which they should exhibit their tables. Twenty years and more have elapsed since that time, and a general Congress has not yet been held. From this it appears that the late M. Kreil, the intelligent Director of the meteorological observations in Austria had good reasons to affix three notes of interrogation to my request.

Something was done by Dove in 1863 at the Congress of natural philosophers of Switzerland at Geneva. He had invited some meteorologists from Italy, Austria, Spain and France, that they might confer at Geneva, thus giving his approval to this means of establishing uniformity in the direction of the transactions, but several distinguished Savants, whose opinion I had wished to hear, were prevented from coming, and thus it was not an international Congress. Dove however himself stated at that time



openly, that little utility could be expected from a Congress, unless the points for discussion had been previously suggested, and this I will try to effect in the following pages.

Several Meteorologists have shared in the wish expressed by Dove. How is it then that nothing has been done in this direction?

The utility of a Congress cannot be doubted, especially if attention be previously drawn to the most urgent questions.

We may state this, since even the Meteorological Congress held at Brussels in 1852 on the claims and interests of navigation and the systematic arrangement of observations at sea, has given a great impulse to the establishment of Meteorological Institutes in England and on the continent of Europe, by which navigation has been shortened and hints have been given to foresee dangers and to escape them. Even in Holland (1), where money was already allo-

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(1) Even in Holland. I think I have a right to say so. There was much prepared in our country. I had strongly addressed myself to the Koninklijk Instituut van Ingenieurs; To the Bataafsch Genootschap van proefondervindelijke wijsbegeerte, to some private persons who trusted to me already a number of extracts from shipjournals, from which I had computed in 1852 the best points to cross the equator. The prefaces of the: *Meteorologische waarnemingen in Nederland, 1848 en 1849*, published in 1850 by the Provinciaal Utrechtsch Genootschap voor Kunsten en Wetenschappen, of the: *Meteorologische waarnemingen in 1851* p. VI; the annuary of 1852 p. 147, 207 afford abundant proofs; and most of all the grant of 5000 guilders allowed to me by H. Exc. the Minister of the Interior, Mr. Thorbecke, on his visit to Utrecht in 1850, whose perspicacious mind was at once persuaded, that the inquiries of the ce'ebred Maury must needs be supported by our navigating Countrymen.



wed to me for extracting Journals, but where an Institute was not yet founded in an official way, I think that the fact of a Congress corroborating what I had proposed, gave a strong impulse. The only reproach that can be made to this Congress is, that too many observations were asked for, more than seamen were accustomed to make, at unusual hours, and many of which f. i. psychrometrical observations and of the density of the seawater, cannot be expected to be made with sufficient accuracy. Now this is just what we should avoid, and be careful not to restrain the liberty of the observer. Every one must consider the usage prevailing in his own country and the extent of his means; we should remember that one who is ordered to observe certain regulations does not effect half so much as one, who investigates according to his own judgement. Liberty should be respected everywhere; but especially in science should her spirit guide all our inquiries. By some very general indications they may be directed in such matters, that the general utility be promoted by the cooperation of all; only such restrictions should be made as are necessary to contribute to the interests of science, to promote the common welfare, by proposing some rules and discussing them. By this means we may obtain a general method to be fol-

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But still I may doubt, if but for the exertions of Captain Jansen of the R. D. Navy, who after repatriating in Juli 1852 addressed himself to the most influential Merchants of Amsterdam and Rotterdam and but for the moral influence of the Congress, the Dutch Royal Meteorological Institute would have been founded on the 1 Januari 1854.



lowed in making our observations and in publishing them.

If our proposals be good, they will be adopted, rectified, if wrong; perhaps others will be recalled from error, more able to give in their contributions in an economical way, and such being our opinion, at all events, no one will be afraid that we are inclined to regulate too much. Our propositions may be defective, but others will complete them; and their peculiar views may throw a light on the same subjects or other inquiries of high interest; only it would be so much the better, if, besides that, they complied with the general regulation.

If we now ask what are the most extensive wishes of science, the idea which we long to see realised, it is without doubt this, that all observed phenomena everywhere, at all times, continually noted down, should be collected, discussed, worked out.

#### § 1. *Number and disposition of observatories.*

Such a plan can evidently not be realised. We have only to think of the expense of printing the complete set of observations of every place, spread over our globe. We shall soon see that every observatory can not be provided on the same scale. Only a few of them will minutely observe all phaenomena. The question will soon arise: at what distance, must we have such observatories of the first class, if we fill up the space between them by observatories of the second and third order, where numerical observations are made, but not to the full extent, or where attention



is only paid to extraordinary phaenomena, to wind or rain. Much depends on the physical features. In mountainous districts without doubt many stations more are wanted. Switzerland, Austria have a great number of stations. But in countries as the Netherlands and Belgium, I should think that only one central observatory would suffice, if it is seconded by other observatories such as Flushing, Maastricht, Helder, Groningen, where hourly observations are taken, and others viz Maastricht, Breda, Hellevoetsluis, Assen, Leeuwarden, where numerous observations are only made at longer intervals. Especially, if we remember that at several other places rain, hail and thunder, are observed. If the number of stations is too great, and their journals too numerous, we get facts compiled from them in nearly as difficult a way, as when we derive them from our own personal observation of the atmosphere, with only this difference, that the phenomena of the atmosphere are fugitive, varying every moment; where as in our registers we can find them for every point of time we require them.

Therefore Meteorology confident, that at many places observations will be made, requires only that they are regularly and explicitly made at stations fifty leagues removed from each other. In England there are seven normal observatories. They are quite sufficient, with one in the north of Schotland, on the Shetland Isles and perhaps, if in addition to these there was one on the Isle of Wight, on the Southside; especially because the number of observatories of the second order is so great and since ample mutual communication is made. The Congress will have to inquire, if that



distance should be reduced to forty, but I do not think so, for when you examine the Dutch annuary, and see the simultaneous departures printed on the same horizontal line, you will not see a great difference between any two places so far removed. According to this scale the number of observatories in Europe may be reduced considerably, even when we allow three times the number to Bavaria, Austria, Switzerland, Spain, to be distributed at these distances and some more on remarkable mountains tops.

In determining that distance the Congress will not disregard the advice of the Director of the central observatory of every country. He knows best, where he will easily find observers of the second order, how those should be disposed in connexion with other established central observatories in adjoining countries, and whether the situation of such a place is particularly adapted for the purpose. Such a station we will call fundamental places, and express our wish, that a part of the money meteorology can dispose of in each country shall be applied to the establishment of such fundamental places in distant uninhabited regions. This would be preferable to a plan devoting all our means to the augmentation of the number of observatories in our own country, which certainly would be more agreeable to our patriotic feeling than conducive to the general interest. Then Europe and America are sufficiently, I dare say, too abundantly provided with observatories. We don't learn more from a thousand of them than from a hundred; but Africa, Asia, Australia, all parts of the world, which are in many respects so interesting, are to a great extent



deprived of observatories. Dove, who for half a century has collected all those observations of the simplest character, was often compelled to be satisfied with a series of only some scores of months. Shall we not be eager to afford to him series of scores of years. The sooner we begin with observations at such places the sooner we shall be able to compare these normals. So was in 1851 my reply to the objections of Mr. Glaisher and Lamont as they begged me to observe, that my calculation of departures was premature. And now I am very content that I began at once, for by the aid of others of all countries I have gathered now a great deal of information for Europe. But what is the case in other parts of the world?

Africa is uninhabited for the greater part; France could give us the northcoast, England has done much on the South point, and along the Suez-canal some stations have arisen. The continent of Asia is entirely left to England on the South coast and to Russia, which centralises her vast territory. In the Isles of Asia there are observations of Spain, Portugal and the Netherlands Dr. Bergsma will try to do for our colonies what has been done for our little country, whenever he has obtained the means required for that purpose. Australia, where the English flag floats has many observatories, but on the northern confines it is uninhabited. America has a long series of observations in the northern part of it; we only want them to be combined, that normals may be derived by comparison of the results, and in the latter years some observations have been collected in the highly interesting Journal of



M. Jelinek and Hann. The observations on the ocean are discussed by England, Holland, and the Institute (Seewarte) of Hamburg. Shall we not follow the example of England establishing observatories where there is the greatest want of them? Shall we not enable Dove, and all who take an interest in general disquisitions, to have records from all places from which we had hitherto no observations, or too few, leaving unknown the climate of whole districts of the earth? Especially in the extreme climates it is necessary to be able to make some comparison of a few consecutive years with the same years at well known stations, if we will not be exposed to draw conclusions, that might prove false and only applicable to those abnormal years, not to the true average. <sup>(1)</sup> Therefore the whole of the globe must be covered with a network of observatories, where observers placed at equal distances are able to watch all phenomena of the weather as accurately as possible.

No doubt many meteorologists have thought the same and communicated that thought to others, which I proposed <sup>(2)</sup> at the time of the establishment of

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(1) Every one will remember in how inconsiderate a manner is concluded from only some fifty years of observation on a change of climate, from a couple of moderate winters in high latitude on the temperature of the arctic circle and within it, from a large space of seawater cleared of ice by winds on an open polar sea! It is a little more probable as the dark cold body of the sun and even that hypothesis I have inconsiderately adhered to.

(2) See my paper *Pogg Ann Ergänzungs band IV p. 550*, where I alluded to the same subject, and where I gave originally the first charts from the Dutch annuary 1852, exhibiting the progress of temperature in Europe; later M. Le Verrier has published from the



the R. D. M. I. when they stated the question: which is preferable, observations at a thousand well disposed observatories for two or three years in addition to more years at some of them, or a series of hundred of years at a dozen stations? We prefer the former alternative.

This proposition appears quite true if we regard the circumstances, happily already realised, viz. that we have several points of the globe, of which we know perfectly the average indication of temperature and barometric pressure, of rain and wind direction. How accurately from this we may conclude on the average phaenomena at neighbouring stations is fully proved by my *Marche annuelle*: normal tables; <sup>(1)</sup> for the results have been proved to be firmly established. Some difference is to be made for places out of known regions, and for those within. Luxemburg is within a short space of time inferred from Brussels, Maastricht, Paris, Treves; on the contrary Haparanda requires more years, not only because the climate is so extreme, but in the first place, because it is not so near to any place of a long series, thesed being moreover aleon one side. Even the Azores, blessed by a moderate climate, a spot in the ocean of great interest,

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beginning of 1858 his more ample and continually extending charts of simultaneons phenomena of the atmosphere. Priority as I acknowledged, belongs to M. Espy for the U. S. and after his example Capt Maury was the first who tried the same thing for the sea. Now after an interval, Commodore Sands has renewed, aided by the telegraph, communicating and exposing the simultaneous meteorological phenomena in the U. S. on a large scale.

(1) *Marche annuelle de la température et de la pression atmosphérique en plusieurs lieux de l'Europe 1861.* Verh. van de Kon. Akad. v. Wetenschappen te Amsterdam. Deel IX.



will soon be known for this reason only, because we are already in possession of six years of observations at Madeira, former observations at that Isle, and a long series at Lisbon. Suez and Port-Saïd require a longer time, since they can only be brought in relation with Constantinople, than a place in Boheemia, between Vienna and Prague, the fundamental places of Austria. No Director of a fundamental place will, considering this, omit such communication of average values of all the instruments for the year for every individual month, for shorter periods, pentades for instance, as they result from former observations, reduced to the same hours of observation and to the same instruments, at the same place, in order to facilitate comparison for every lapse of time.

§ 2. *Number of observations to be published.*

So much about the number of observatories. Moreover we are of opinion that the central observatories should not publish tables too detailed.

Indeed we do not clearly see that all these tables should be necessarily printed and published, as is now generally done. It is an agreeable luxury, if at every station hourly observations are recorded, if, as in England it is so fully and royally done, the range of each instrument and its operations and continual changes are communicated; but still it is luxury.

Every one wants these tables at his own station to inquire duly into the mutual relation of the simultaneous changes, but even though they are not printed, we grant that, if we have to determine



the velocity with which a barometerundulation, a change of temperature or a gale has traversed the continent, continual observations are wanted; especially for magnetic disturbances do we need (1) them; because we must know for every single moment of time, what has happened at other places of the world, if we wish to decide, whether cosmical or telluric influences have been the cause. But even for these inquiries, we ask with confidence: is not it quite sufficient, if we may consult them from the original, is it requisite to print them by thousands, when perhaps no one uses them?

In this respect it would be preferable to have the wind observations printed, as we shall prove more amply afterwards, because every one is more eager to understand the signs of coming winds and the propagation of those actually blowing. But the thermometer- and barometerchanges are more equable and gradual, and generally the knowledge of their indications at any time, at several places, enables us to conclude with great accuracy on their height at a neighbouring station.

When we look at the observatories of the second order, it is quite sufficient, if they give a record of their observations, which may be very explicitly printed on one sheet of paper, for a whole year; perhaps the

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(1) See my paper in de Versl. van de Sectievergaderingen van het Prov. Utr. Gen. 1862, where I showed, on inquiring the limits of ordinary and extraordinary disturbances, that even hourly observations conducted by Sir Edw. Sabine at the stations Toronto, St. Helena, the Cape of Good throepe, Hobarton are quite insufficient to decide on the simultaneity of the disturbances all over the world.



observer can persuade the editor of the local newspapers to give a separate copy of those daily communications, and to collect them every month, as we have often proposed, and to have some copies sent to the observer, that he may disperse them. In this case the charge will not be heavy, which is a desideratum; but on the whole, we must be very economical in printing, because there is so very much that must be communicated.

To the observatories of the third class we leave the observation of rain and accidental annotations of hail, thunder, gales, changes of the direction of the wind, parheliæ, aurora, shooting stars etc., which require only the exact definition of time and direction, but no instruments.

When we assigned in the first place to the observatories of the third order, the observation of thunderstorms, it was not because we judged them of less importance, but because these phenomena do not extend over a vast territory; they might escape the attention of the observatories of the first class and these could by no means well study their line of progress. The highly valued disquisitions of M. Le Verrier in his *Atlas des Orages*, and those that are communicated in the Dutch annuaries with precise definition of the time, at which they appeared, are all been made possible by these observers and amateurs without any instrument. The same may be said of the observation of hail and great rainfalls. In all these things every one must act with perfect liberty: emulation and a few directions will teach what it is best to be done.



If, on the contrary, the annotations are duly registered, we have always full time to discuss and prepare for inquiries of a general kind. For my part I have always experienced the greatest kindness from observers of every country. I therefore cannot be grateful enough, and feel compelled to return them my heartiest thanks, now only in words, and afterwards in facts, if for the future I try in the same manner to answer questions directed to me relative to weather phenomena, occurring at a time, of which the observations were not printed.

It is not to be doubted, but that everyone will be inclined to give information as to a special observation, if it be wanted for any inquiry; the more so as the investigation is of higher value and interest. I venture to ask, if Prof. Mohn, Dr. Buchan, M. Wojeikof, and others can complain, that for their interesting study of storms any information was refused? Neither have they refused it to others, as I have often experienced myself. From time to time we send autographic copies of the magnetical observations to other observatories, to Kew, Edinburg, St. Petersburg or to anyone, who investigates the laws of perturbation; as to Mr. Diamilla Muller at Milaan, when remarkable disturbances have occurred, with the intention to inform every one, that he can obtain every communication from Utrecht, if he wants it; so the outlay is not large for our Institute, and nevertheless every want is supplied. Is it then necessary to print all these observations, for every place, from minute to minute, when even this would be insufficient, as we have just shown? Even Greenwich shrinks from such an expense



and gives only the times and values of maxima and minima, and if England, where all is conducted in such a liberal way, cannot master the mighty stream of observations, no one can try to do so with any hope of success. Here we must record the brilliant publication of M. Rob. H. Scott, who gives in continual lines at two opposite pages the indications of every meteorological instrument at his seven stations, simultaneously. The inspection of these charts enables us to see in a moment, how one of the instruments was affected, if another showed a change. When we consider, that, to get a more ample view of the progress of these changes from England to the continent of Europe, we want moreover the translation of these lines in ciphers and the Juxtaposition of the values of those at other places, we feel sorry, that this splendid publication requires an outlay of many hundred pounds sterling a year. Indeed, when we attend to the paucity of observations, which we get from other extensive countries, we must not indulge in luxury, and it would be better to spare something, in order to erect therewith observatories in vast regions of ice and far from civilisation; were it only by placing registering instruments, as has now been projected for Spitsbergen and Groenland, or at the top of mountains.

### § 3. *Invariability of chosen hours and instruments.*

Now we shall have to on speak another point: on the invariability of the hours chosen and the disposition



of the instruments. A change of instruments is not always to be avoided, they may be deranged or broken. When this happens, we cannot but replace the broken instrument by another. If it is simply deranged, we ought to compare for a space of time the instrument with the new one, destined to supersede it, and because it may be speedily deteriorated, it would be best to have always a second set of instruments ready, whose indications are compared every month. When hours are changed frequently we are in a less favorable condition, for we are not at all justified in concluding hastily from the observation of one hour on those of another. Even when by registering instruments hourly observations are taken, we are not certain, that in other years the range of the instrument, especially of temperature, is the same, as it was in some former years. <sup>(1)</sup> Through this the value of former observations is very much lessened, which I have developed more amply in my: *Prière à ceux qui veulent bien de la Météorologie*, corroborating it by examples. The difficulty is quite the same, if we change an hour of observation, as it is, if the climate of a place is changed by extirpation of a forest, or by digging a canal or a lake, as between Port Saïd and Suez. When the Harlemermeer was drained, it was not more troublesome to me to appreciate the elevation thereby of the

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(1) We learn by a letter of M. Hildebrandson just now received that M. Rubenson, agreeing with M. Svanberg lays a great weight on this point. as generally more and more prevails. The paper of M. Dove: Ueber die täglichen Veränderungen der Temperatur der Atmosphäre, facilitating this reduction for many places, dates from 1856.



temperature of Harlem and Swanenburg in summer, and the depression of it in winter, by comparison of five years before that period with the same years at Helder, and of five years after that period with five other simultaneous years at Helder, as it was to fix the difference of the temperature between 6 and 7 in the afternoon at Maastricht, or the difference between 8 and 9 o'clock Utrecht and at Helder. You may see from the annuary for 1858, how great the difference of range between these hours was in different years; so great, that I at first presumed, that there was a period depending on the period of sunspots and of the declination of the magnet. I often felt compelled to add the Greek letters  $\alpha$ ,  $\beta$ ,  $\gamma$  to the names of the places, as chemists do to the signs the elements, in consequence of the different combinations of the hours of observation <sup>(1)</sup>.

In this I did not pay so much attention to the fundamental places, because I suppose, that at these places hourly observations will be made, but more to the observatories of second order, which deliver their results to the Director of Meteorology in their country. They may choose the hours most conveniently; but when chosen they must be conscientiously adhered to. For it would give great trouble to these Chief Directors, if they had continually to change and to derive previously their corrections to be applied to the results, before they could publish them and

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(1) Sur la Pression moyenne de l'Atmosphère en plusieurs lieux de l'Europe. Verslagen en Mededeelingen van de Vergaderingen der Kon. Akad. v. Wetenschappen te Amsterdam. Vol. XVI: 1864.



regard them as contributions to the knowledge of the climate of their country. For others, as the Directors in the country itself, who at all times can get informations, who know the locality and can give rules and directions for the disposition and calculation of these observations, it is quite impracticable, and the Director of Meteorology in that country must not too much be burdened with these corrections and diverted from pursuing the great object in view: the progress of phenomena over the globe! Even for the fundamental places it is not superfluous to observe, how necessary it is to keep true to the hours once selected; for, if as we noted above not the whole of the observations will be printed, they must take care to select always the same hours.

§ 4. *Astronomical or local time. What hours?*

After having stated the necessity of the invariability of instruments, of their disposition and of the hours once selected, we proceed to the examination, at what hour and how many times a day the observations shall be made, that are to be printed.

If a combination of hours is to be selected, the first question arises: shall we take them at astronomical hours, simultaneous moments all over the globe, or shall we prefer local time? It is not superfluous to take this into consideration.

Arguments of great weight may be offered for the former alternative.

If we suppose that the observations will be given three times a day, which we shall find recommen-



dable, at intervals of eight hours, we should get very correct notice of the distribution of atmospheric phenomena over the whole earth, at the same point of time. By this means it would at the same time not be necessary, in most cases, to ask for further communications about the observations of barometer and thermometer made in the interval.

From a theoretical point of view it appears highly recommendable to do for other atmospheric phenomena, what has been done by Gauss with respect to the study of the magnetism of the earth with great success. No one can reject this but for practical reasons and objections.

The objection that this image so obtained would be confused and falsified by the daily range is not of great weight with me. Only this concession I can make, that the image would be confused, if only departures were given, without indication of the normal values from which they are taken. When however these are added, all mistake is impossible. Departures are perfectly independent of the daily and annual range, and of the local disposition and correction of the instruments, because the normals are likewise computed for these circumstances. If we inspect the annuaries of the D. Met. Inst. and look at the departures at 8, 2 and 10 o' clock separately, it will be found impossible to see, to which of these hours the numbers belong, but from the head of the column. The daily period has wholly disappeared, only the true value of the disturbances has remained. Practice has fully shown that this objection is of no great moment.



Another objection opposed to this, might be thus stated: when the daily period disappears owing to the use of departures, of course we cannot learn it from them, as we should like to do, were not the normals given at the same time at the head of the column for each place. But for this very reason they are given. By adding three numbers for the normal values, for the fifth, the fifteenth and the five-and-twentieth of such a month, it is easy enough to ascertain the normal values of each of the intervening days with a possible error of only a fraction of a degree.

The objection does not hold, as we see, for the places, where the hours, 6, (18) 2, 10 are chosen; but even when for a place according to its difference in longitude the hours, 10, (22) 6 and 2 (14) ought to be taken, the most removed from the hours indicated, the differences resulting for the mean value of the day, in the twelve months, are not so great as you might be inclined to suppose. Calculating the results of the hourly observations at Utrecht, as they are found in the annuary for 1858 p. 226 and for 1868 p. 262, I find the corrections for 14, 22 and 6 (1) o' clock for the different months thus:

|          |        |           |        |
|----------|--------|-----------|--------|
| December | — 0,05 | June      | + 0,12 |
| January  | + 0,08 | July      | + 0,20 |
| February | + 0,01 | August    | + 0,26 |
| March    | + 0,07 | September | + 0,04 |
| April    | + 0,18 | October   | + 0,05 |
| May      | + 0,16 | November  | — 0,03 |

and for the barometer, of course, much less.

(1) I must observe that two errors have crept into this last page, happily compared to each other, since September 10 o' clock has not 182 but 282 below the average, and 10 o' clock not 261 but 161.



The errors to which the average of a whole month is liable, and properly said we have only to meet the differences of these errors, are for a place a great deal less than the error made by every one, when he takes the mean value of the average of a combination of observations for the value of a single day. A couple of objections of minor importance are these. The time of taking observations may be very inconvenient for such or such a place; but this is of no account, because we speak only of observatories of the first class, whose continual observations are made by registering instruments. A great many difficulties may again arise about the place, that would appear to have a sort of preference, being indicated as that, whereon the time of observation is regulated. It is the old question, if Rome or Carthage shall impose their sceptre on the world; we hope however that no such impediment, like that of the diverse weights and measures, will present itself to excite competition between Berlin, Greenwich, Paris, St. Petersburg, Washington, places from which the longitude at sea is reckoned, and where at the same time so much has done been for general Meteorology; for we may answer readily: if we do not choose among one of those places known for Meteorological labours, and the Isle of Ferro, where nothing has been done, when narrowmindedness throws down the apple of discord, instead of leading to conciliation and mutual help, let the ballot decide.

Yet advocates for the other method of observation may fairly insist on their opinion being taken into consideration.



They may allow that observations at one hour cannot teach us the daily range at a given place; it may be alleged that nothing is proved by many of the combinations made in past times as at sunrise and sunset, varying continually throughout the year, even when an observation at noon or two is added; they may call the French and Italian manner of selecting the hours, generally during the time, when the sun is above the horizon, a defective one; that the hours must not be the same for thermometer and barometer, and that the object is thus very often not attained; yet we believe this party will still gain the cause, since the observatories of the second and third rank would be more closely connected. They will say: if we concede that the hours actually selected are not conducive to the purpose, we will select others etc, and we must then consider, which would be the best combination of local hours.

In that case we must decide how many observationhours shall be adopted as a rule, and at what time. If only one observation hour be adopted, the breakfast hour is most recommendable for its convenience; or 7 o' clock, for the concurrence of its indications with the average of the day (*Jaarboek* 1858 p. 226, 1868 p. 262). More than twice as good is the combination of two hours, especially of the so-called homonymous hours 6—6; 7—7; 8—8; 9—9. If needs be, two hours might be selected later in winter and earlier in summer, in order to learn from them with tolerable accuracy, the mean variation of the day. If we would learn rather the variations in the temperature than the mean, four o' clock in the mor-



ning (16 hours), and two in the afternoon should be selected.

Even if observations are taken twice a day, an evening and a morning hour should not be selected unless it be to agree in one hour at least with all observatories, but rather the hour of the highest and that of the lowest temperature, as these may just form a subject of inquiry.

The same object may, however, be attained better and with less labour, if we confine ourselves to the maximum- and minimumthermometer.

We then get the precise range of temperature in half the difference, and pretty nearly the average of the day in half the sum of the observations with only one observation of that instrument at almost any hour. Kämtz has already been pleased to determine, how much half the sum in the different months deviates from the mean, and this correction might be placed at the head of the monthly table.

Only in winter, and where the variations are slight, can any difficulty arise. It sometimes happens, for instance, that the thermometer sinks the whole day. We may ask: what is then the minimum? In such a case, I should propose placing in the minimum-column the highest number observed, and in the maximumcolumn the lowest of that day after sunset, with the sign — prefixed, as we do in the Dutch annuaries. It is then evident to all, when and to what degree the anomaly occurred, and it is not inconvenient for any other purpose, because it has no meaning to take the sum of the mean or of the Maxima and of the minima of each day.



Three observations will be considered the best; a fourth and a fifth afford greater accuracy: but to spare printing costs, space, etc. we must keep within certain limits; and though we may say there is a decided gain in accuracy, it is in a smaller and smaller degree by one new observation more, the more we have of them.

For the threehours combination preference must be given to 18, 2, 10 indicated by Dove, as that which gives both range and average tolerably correctly.

This combination ought to be adopted, were it only to pay due honour to his vast services, and not merely because most observations are taken with it.

#### § 4. *Barometerreadings.*

For the barometer too, this combination of hours is acceptable, for it gives very nearly the afternoon-minimum and the eveningmaximum, though 20, 4 12, would in this case certainly be better. From both the average of the day is nearly known, but with respect to the barometer, it is not for this reason that I propose such a combination. On the contrary, if at this Congress a plea be offered for the plan, adopted in some places, of communicating the height of the barometer for the daymean daily, I will try to oppose it. May we not inquire indeed, whether the average height of the barometer in our latitude is of sufficient importance to be recorded along with the height of fixed hours? The average has no real signification in reference to the pressure, as it has to the temperature. The daily range of temperature is con-



siderable and well defined; therefore we admit that one who does not prefer departures to observed heights would wish to eliminate it, moreover as the same local hour has not the same value with respect to the daily means, in places at different latitudes, where the sun rises or sets at times more removed from that hour; for the barometer all this does not occur, or only in an exceedingly small degree, for the former on the contrary a great fall or rise at a single hour of the day has no preponderating influence on anything whatever; for the latter it is just the variation much more than the mean of pressure that we attend to, and so we do not distinctly see, why we should do for the latter, what we require for the former. Is it not much more to be regretted, that Switzerland has deduced the mean height from three hours, and publishes this mean and besides two observation hours, instead of giving the observations of each of these three hours?

It has been alleged against this opinion of mine, that individual heights are not of equal importance to every inquiry, and I grant to some investigations the average height to be of more avail: but may we not remark that with three heights we more readily deduce the average height from them, than from the average height and two others the third, which has contributed towards finding the average height. Thus we should like to have hours fixed for barometrical observation and these individually communicated.

By mean daily barometerheights a comparison cannot be made between places in distant meridians, because the high reading in the evening at one



place may occur in the morning of the following day at another station, and for the place itself it is just then of no signification, when we desire most strongly to know more of it. Let the barometer have a height of 770 mm. in the morning, 760 in the afternoon and 750 in the evening, the means 760 would seem to indicate a very calm atmosphere!

So even he, who sees any value in the mean barometric pressure of a day, must beware, whichever combination be selected, of overrating any, for though one may be the best for long intervals, for the average height, yet on a certain day it may give inaccurate results. Let everyone but examine his observations of any month and he will find days, on which the thermometric result deviates one degree and the barometric mean some millimeters from the average in twenty-four hours; not only from the less efficient combination, but also from 18, 2, 10. It is thus no relative defect of any combination whatever, but the necessary defect which is peculiar to all surrogates. The real average is the mean value of all the peculiar values of the function, and these can be but imperfectly deducted from conflicting observations.

Besides the time of observation, it will be necessary to discuss how their arrangement shall be. Hence we must again in the first place determine that, whatever we may resolve on doing in the future, for the present, we shall adhere to the method now in use, and continue in doing as we did, for the sake of making comparisons.

Some may be inclined to reduce the observations not only for temperature, and rectify certain known



faults in the instrument, for capacity and capillarity, but likewise for the height at which it is placed above the sea.

The first is necessary, the second is of no moment the last is not allowable. The less the elevation above the sea, the less detrimental the reduction. In this case there is even an advantage to the appreciation of the immediate observations, as by merely deducting 760 we get comparable numbers affording the approximate deviations. Thus we must look upon this endeavour to reduce to the surface of the sea, as a latent wish to ascertain the deviations. It is a pity that it does not give a quite satisfactory means, as the barometrical height is not exactly 760 in every latitude; but more in the tropics, and less at the equator and poles; it is just as imperfect a means of indicating, as some have done, deviations from the average of every month in which observations are taken, since observations noted in this way during different years no longer admit of being immediately compared, nor do they give the true measure of the disturbances. It is advisable to have recourse to some better means by determining the normal value of instrument, position, hour of observation, and site of the instrument, and indicating the deviations even of places the elevation of which is but slight.

What is here barely allowable, is not to be thought of for places a hundred yards and more above the sea, especially in mountainous districts, in the vicinity of high mountain ridges, or on a table land.

It would not be difficult to show, as I have done in my correspondence, that departures of the three barometrical heights in such a place differ much more



from the departures in adjacent places, than the departures at places a hundred leagues from each other differ mutually, and that thus practice condemns, what theory proves is not feasible.

At such elevations the formulæ can indeed no longer be applied, were it only that by different winds the pressure in valleys and on the slope of mountains is differently affected. Were we placed in a tower rising from the midst of the ocean, the movement of the air would be unobstructed, moreover in that case we might observe the temperature underneath, now even respecting this we can but form conjectures: the temperature on the place of observation is different from what it would be were the ground not so elevated. Besides what we formerly stated is applicable here: what are recorded as observations must indeed be such, unaltered, even after a conjecture, however probable.

Only what we know for a certainty, the temperature of the mercury column, may be taken into account, and what is not done, but may be just as well be allowed, the alteration in the gravity. A low barometrical height at the poles is like a higher one at the pole, the relation being well known; why then shall it not be taken into account? In the employment of departures everything is taken into account, this last influence too.

#### § 5. *Thermometerreadings.*

The thermometer is apparently a very simple instrument, the reading of it is easy, but it is not



easy to be certain, that it gives accurately the time temperature of the air.

The thermometers, which can be read immediately, may without much trouble be placed at a distance, and the height of the quicksilver determined with a telescope; but for the self-registering thermometers it is difficult to get a free and thorough current of air. It is a difficulty, which has not been removed even by the best constructed meteorographs: those of Dollond (1851), of Wild (1861), of Secchi (1867) of Theorell (1870); and which causes perhaps more obstruction than occurs with the common selfregistering instruments, as their compactness does not allow a free air. Though the thermometer be sheltered from the sun and the air be admitted through glass blinds at some distance from the walls of the buildings, though we guard against irradiation from other objects, yet may the air continue to linger against the wall, especially when there is any wind from the side of the sheltered wall.

For registering thermometers we prefer those, which give their indications by the different dilatability of a metal and glass. They have great force for moving a balance, and a great surface, that a slight radiation in one part and the named perturbations of obstructed may be compensated in another part of it.

In Utrecht M. Renou on his visit frequently found with his thermometer *à fronde* in the garden of the Observatory and around the town  $0^{\circ}$ , '3 C. less registered than at the Observatory; and nearly the same discrepancy will occur elsewhere. With every other direction and force of wind, this disturbance



is somewhat different, so that this defect is not entirely removed by calculating the deviations. It is here sufficient to have stated, that the different disturbances, as has been proved by M. Martins, Pres-tel and others, depend on the clearness of the sky, the force of the wind &c. Thus only few will direct a parabolical mirror, in the focus of which a thermometer is placed, towards the highest regions of the atmosphere, in order to ascertain, whether in the upper strata higher or lower temperature prevails, and whether that affords any indication of the temperature of the following days.

Corresponding to these observations it may be good to speak on the observations of groundtemperature, and how to make them on a fixed plan. The tunnel of the Mount Cenis affords an extraordinary opportunity for ascertaining temperatures in places separated by thick strata from the surface. The interest in these researches is hereby more awakened and we appreciate more fully, what already has been done at some fundamental observatories, situated on formations, above others appropriated to inquiries of this sort. The observers there will be induced to place their thermoelectrical thermometers as deep as possible, For, if we allow that the study of temperature and its range in the upper strata gives us sufficient information in relation to plants, we are in behalf of the theory of telluric heat very desirous to know what happens in the deepest strata, that we may better ascertain the quantity of heat emitted by the earth and conclude on its refrigeration which has been called into question by Laplace, since he did not pay



due attention to the causes by which the rotation of the earth is retarded.

Other investigations might be discussed, but we will confine ourselves to the general questions. Shall we read also thermometers with a black surface or of some other colour, or had we better use the pyrheliometer for the inquire of radiant heat; shall we have to speak of placing thermometers on the grass, in and without a forest etc?

Is it desirable to absorb through an aspirator the air coming into contact with the thermometer by a well-furnished, non-conducting tube, the aperture of which is placed elsewhere in the open air?

Here too are drawbacks, for however the exterior of the tube be constructed, the state of the moisture may easily be thereby affected, so that the conducting tube may be too long or too narrow. This would occasion inaccuracy, especially in observations with the Psychrometer, and yet I am of opinion, that whatever Psychrometer we may use, an aspirator always be employed with it.

If asked how these readings are to be printed, we answer: By giving departures. It is more necessary for every table designed to give a general view than for the barometric readings. It is worth our while to dwell on this point, as the departures have a special signification with respect to the indications of these two instruments. I allow that departures afford a measure, not so much of the temperature, as of the pressure of the force trying to restore the equilibrium; but on the other hand, the readings for temperature are quite unintelligible. If  $10^{\circ}$  C. is marked



for Moscow, it is unusually warm there in winter; while for Iceland this indication would denote cold weather; on the contrary, the same temperature  $+12^{\circ}\text{C}$ . in summer denotes warm weather in Iceland and cold at Moscow. Thus the temperature observed at different stations in Europe, does not enable us to make a comparison at those places, but the departures do.

It may be alleged against the employment of departures, and this may be said of all instruments, that we get positive and negative numbers, that it gives some trouble to add them up, and that we are liable to make mistakes in the correction and inspection by confounding  $+$  and  $-$ , or, if the mode of printing is followed by which separate departures are indicated by larger type and negative by small ones, by not well distinguishing them. The first objection has no small weight. It requires a great deal of attention to add numbers with opposite signs; and to take them separately, even when they are united in other letters, is also troublesome, if the change of signs frequently occurs. Therefore Dove proposed to use the Fahrenheit scale augmented by 100. The same could be done with any other scale and we shall not object to adding 100 to the number expressing the departures. For in that case the positive departures would be easily distinguishable, since they are expressed by one figure more, all numbers are positive, and their sum during a month of thirty days, diminished by three thousand gives, after division by thirty, the average in degrees or millimeters of temperature or barometric pressure.

Only one reason militates against distinguishing the



departures by the signs  $+$  and  $-$ . In both cases we have to add them separately. The sum of the sums  $a$  and  $-b$ ; or  $a-b$ , gives the mean departure, and their difference  $a+b$  the mean variability; while after augmentation by 100 we are still obliged to ascertain how often departures of each kind have occurred.

#### § 6. *Psychrometerreadings.*

We have already reminded our readers that it may perhaps be desirable to use an aspirator for the readings of the thermometer; for those of psychrometer it is decidedly indispensable.

It is already used with Regnault's and with von Baumhauers <sup>(1)</sup> Psychrometer. The latter, a glassvessel floating in oil, is preferable to the former, as it registers continually, in order that the average moisture between every two points of time desired, may be ascertained. A constant current of air is conducted by an aspirator, caught by that floating body in which there is chlorcalcium, and measured.

Thus we know how much air is carried over that chlorcalcium in a definite period of time, and how many grams of vapour have been condensed by it during the same interval. From the increase in weight visible from the greater depth to which it sinks, we immediately get the quantity. It is requisite however, that the oil should have a constant and

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(1) Perhaps the idea was suggested by Prof. P. Harting. Transactions at the meeting of the Sections of the Utrecht Society 1849; but the instrument was constructed shortly afterwards by Prof. von Baumhauer.



somewhat higher temperature than the external open air, while the conducting tube must be somewhat higher in temperature, in order that condensation of the vapour may not set in too soon, before it has arrived at the chlorcalcium. But neither is the psychrometer of August, without an aspirator, according to the minute investigations of Regnault, a comparable instrument, when the draught of air is not known and constant. Therefore I have made some alteration in it at Utrecht, by placing both in the two parts of a cylinder, of not too small a diameter, separated by a circular glass plate, which only at the circumference admits the air to the wet bulb, placed in the latter half in the axis of the cylinder, after it having passed the dry one. An aspirator causes the current of air to be always equally strong.

The question may arise, whether it is not of more importance to know immediately the quantity of water, than the tension of the vapour.

In favour of noting the tension, which has hitherto always been the custom, it may be stated: that Dove has explained from it the daily and annual variations of the barometer. The variation of the pressure of the so-called dry air appears much more simple than that of the atmosphere, subtraction is easily made; but yet there are good reasons for being cautious in applying this mode of separation of the two atmospheres in particular cases.

We believe, indeed, that the vapour would form an atmosphere of its own, if it had time to attain a state of equilibrium; but it cannot be denied that for this purpose sufficient time is not allowed. The



experiments of Lamont show it, and observations of diffusion have taught us, that a considerable time is required for mixing two gases according to the law of Dalton. I do not recur to electric influences of sun and moon, for agents in causing the daily variation, I say only: What remains after deducing the pressure of the air, is not purely the pressure of the dry air. It may be dry below, and nevertheless the sky above may be covered with clouds. On such a day, we should thus, after deducting that slight tension, retain too much pressure for dry air, as all the water contained in the clouds weighs hydrostatically, and not by tension as vapour.

If below a dry air passes through a column of vapour, and divides it into two, the upper part, owing to its tension, ceases to operate on the lower part, and the tension of the lower part may at first have remained the same, yet we are sensible, that the *layer* of vapour divided by such currents of air, no longer works correctly on the psychrometer beneath. Everything above us in the form of nebulous or falling drops acts as floating or sinking bodies only by the weight, in the latter case only modified by the acquired velocity in descending. May this be disregarded in so much as it relates to our actual question, it is certain, that not these upper particles of water, but the quantity of vapour beneath exercises an immediate and great influence on the health of man and the growth of vegetation, which is thereby more accurately indicated than by the relative moisture.

The vegetation and the soil always condense vapour,



though the air is not saturated with it (1). We grant that this has been hitherto decidedly underrated, we allow moreover that they do this less in proportion as there is less moisture, and are not disposed to cease recording the tension, but it may still be deemed desirable to mention along with it the average quantity of vapour in grams; though it were but read once a day. It may be that by the above afforded reflections, we shall not prevail in lowering the appreciation, in which the psychrometer is held, by no means the Congress will be inattentive to the following remark. The tension of vapour depends on two influences: both on the quantity of vapour and on its temperature. Principally by the latter it is a very variable element. In a few minutes of time, tension may vary very considerably, the quantity of water being the same, and on the contrary quantity may increase, it acts not very differently on plants and animals, if simultaneously the temperature becomes more elevated. Besides this we are already accustomed to give relative humidity likewise depending on the two circumstances mentioned. Now we think it will be sufficient to print only once that complicated effect, in communicating either the relative humidity or the tension, and not twice; but to give, instead of one of those forms of the moisture, the absolute quantity of vapour, as is best done by the instrument of von Baumhauer.

#### § 7. *Raingauges.*

From the psychrometer we proceed to the raingauges

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(1) See the interesting inquiry of M. Dufour on the vapour condensed by glaciers 1871.



The form of the upper aperture is, as proved by experiments made at Utrecht, a matter of little importance. Two raingauges, one having a circular aperture, the other a square one, were long compared.

Only at places very much exposed to the wind, does the quantity caught in a raingauge with a triangular aperture, differ, according as one of the points, or a flat side, was turned to the wind, — but no one will select such a form, except for experiments. I suppose then, that the form recommended by Dr. Symons will be preferred. Moreover his rules should be followed in choosing the height of the raingauge above the ground.

Some report the amount of rain, others the height, to which the rain fallen would cover the earth, if it remained on the ground.

We have only to decide what rule we shall universally adopt. To determine with what velocity a shower of rain moves, it is not superfluous to know exactly, at what time the rain began to fall — and when it ceased. If this be effected as at the observatories in England, and at Utrecht, where the receiving vessel is suspended from a balance whose stroke is registered, we may thence compute both the average density of the rain for any period desired, and, by comparison, the space of time that a rainshower arrived later at another station. It would perhaps be better to have the volume of the fallen water registered, as is done at many places in Germany, but in whatever manner it may be done, we shall be able to deduct the distribution of the rain according to the different hours of the day. Before we speak on this point, it may



be asked, whether for every direction of the wind, we shall specially note the amount of rainfall, which accompanies it.

The receiving cylinder would then require a lateral tube, the lower aperture of which should be moved by the wind itself above the sixteen sectors indicating the directions of the wind.

In a little work, entitled: Wenkebach's Observations at Breda, I proposed to proceed thus, not to say: so much has fallen by S W, but so much has fallen when the S Western direction of the wind, during the shower, veered to the West or to the South, or whether it remained nearly stationary. Observations were afterwards taken at Utrecht, the results of which are recorded in the annuary of 1868, and evidently show, that, as we expected, it is not so much the special wind that causes rain, as the variation of the wind mingling two currents of air of different temperature and capacity for vapour.

There are still other investigations, as those of the size or temperature of the falling drops according to the height above the ground, bearing a more special character.

There will, no doubt, be some observers who will avail themselves of the opportunity of a high tower or a hill, in or near their town to make this inquiry. At Utrecht Prof. van Rees has directed and collected observations, made during four consecutive years, of the amount of rain, at the top of the Cathedral tower and beneath. Raingauges were placed at the four corners of the tower in the height of 100 meters, from which it has been found, that much depended on



the exposition to the wind. A greater amount fell on the sheltered side than on the exposed, where the drops owing to the condensation of the air against the building, dashed over the raingauge. However not so much was collected above as beneath, which is to be attributed probably to the above mentioned cause viz, that the rain is not brought by the wind, but partly made between top and bottom by the intermingling of two currents of air.

In other countries raingauges may teach us the so much disputed influence of forests. In Silesia the plan has been made of placing them at distances of a few miles on all sides of a forest, to see if the average is the same near and at a distance from the trees.

Even after the experiments and able inquiry of E Risler I think it necessary to determine on a large scale the effect of vegetation. So I sincerely thank Mr. Dufour for his interesting paper on condensation of water by glaciers, the amount of which results from the mutual comparison of the product of raingauges at surrounding stations, with the quantity of melted ice pouring from them. These researches are not to be overlooked by the Congress; nor is the question to be despised.

§ 8. *Hours for reading the raingauge.*

Is not the rainfall greater, when the temperature is falling than when it is rising? Greenwich and Utrecht have answered this question affirmatively. This difference we have found to be so slight, that we



need not change the hours in the course of the year, but might very well adopt six in the morning and seven in the afternoon in every month.

With a view to this investigation, we might determine on drawing off its water three times a day, viz, at seven in the morning, at two in the afternoon, and at eleven at night, in order to ascertain the rainfall during a period of increasing, decreasing and stationary temperature. In determining the amount of snow, we require, of course, two meters; to register hailshowers with accuracy, we might avail ourselves of a construction similar to that invented by Dr. Krecke at Utrecht, where in the bottom of the first funnel a metal sieve has been inserted, in which the solid parts immediately weigh and leave a mark, and, afterwards melting, flow down into a second funnel, and are then measured again as water. The amount of water exceeding the hail, is the water which accompanied the hail, and which unfortunately cannot be distinguished from the water, which fell during the hailstorm. The instrument is not quite exact. To determine the size of the hailstones, we have merely to take up a certain quantity of hailstones with a pair of nippers and then weigh them. This should be attended to at the smaller observatories. Their internal structure may then be immediately examined.

It is not necessary to regulate the time of recording the quantity of rain according to regular barometrical fluctuations, as we have done according to the variations of temperature, because the influence is too slight. For prognostics, an inquiry, such as we have made: what is the probability of rain and its quantity for



the following day, if the barometer has such and such a height to day, and varies so much, — is very interesting to practice; but to obtain this, we want a special investigation, not an a priori indicated time of collecting the rainwater.

Hence on this point no rules need be laid down. An other question in this. An hour should be fixed for drawing off the water in the observatories of the second and third rank, which must be provided with a raingauge; for this instrument in its simplest form, is to be had at so little expense, that, after the example of Buchan and Symons it may be felt desirable to spread it far and wide.

If the water is drawn off once a day, it is perhaps better to do so in the evening, for we have then the rain of the same day, the date of which is recorded. I at least should like to adopt the change here suggested, for this reason, and because I find I am liable to err, if I have to bear in mind, that the rain I draw off to day, fell yesterday, whenever I mention days of great rainfall or the manner in which the rain proceeded from one country to an other, and when all the days of the month must be enumerated; for then and everywhere other dates must be given than these, on which the quantity of rain is noted.

From this we shall now see as we showed for Utrecht in the annuary of 1868, how much greater the probability is, if it rains, that it will rain also the following day, and if it be dry, that it will be dry the following day, than that the contrary will occur; and after a larger lapse of time we shall be able to ascertain the difference of that probability for different



places. If we draw up tables for all stations spread on a vast region, recording the rain for each day, we have as in the Dutch annuaries a splendid oversight over the distribution of rain, and we are able to determine in a short time, by simple additions, the influence of the moon's phases, not as Matthieu de la Drôme and others dream of, but variable in the amount depending on the temperature of the moon, varying with the illumination of that satellite, and therefore communicated in a varying degree to the earth.

#### § 9. *Atmometer.*

Probably animated discussions will be held on the instruments for measuring evaporation. What sort of atmometer is most to be recommended for the indication of the quantity of water evaporated from the surface of water? Shall we use the common one, or those proposed by Viviani, Lamont or Prestel? How large must they be?

On this subject some regulation should be made as a larger atmometer, of course, indicates less, because that part of the air is always most saturated, which has partly passed over the atmometer.

It would have to be very large indeed to indicate with the strictest accuracy, how much of a pond, a canal, or a lake, evaporates. If we make no further inquiries, we learn merely a little more about the relative moisture of the atmosphere, the influence of sun and wind: if we would arrive at practical conclusions, if we would ascertain, what power a machine must have to drain a lake and to keep it



dry, we should likewise expose sand, clay and ground covered with vegetation, to evaporation.

The evaporation from ground covered with vegetation will in dry weather be greater than from water, as has been proved by a series of observations, and less in damp weather, because it then condenses the water from the atmosphere, even though it does not yet rain. The influence of circumstances should be known. Unless more minute observations be taken, we shall certainly get but a faint notion of what is restored to the atmosphere. The amount now seems larger than what is received. Yet on the whole this is positively inconsistent with the fact. What would become of the rivers of England, if more was evaporated than what falls. It may be right in lower regions, where in general less rain falls; yet even here it will not be taken for granted. The amount of water carried off by rivers does not even indicate how much more falls than evaporates, for a part of the water passes through the ground into the sea. What method of observation shall we adopt? Shall we lay an impervious bottom over a considerable surface, and put thereon draining pipes, to determine the amount of water carried off? or shall we enclose a large space of ground in a cistern and weigh it. The latter method is preferable but more difficult to effect. We have an apparatus for this purpose at Utrecht, but we have not yet taken the weight with sufficient regularity.

This instrument is to be read the same hour or hours as the rain-gauges



§ 10. *Anemometer.*

Now we come to the observation of the wind. Direction and force cannot well be given by departures, certainly not direction. Departures have no signification as for temperature and pressure, since the values of the latter give a measure of the disturbing forces, not the former. Moreover it is not sufficient to give only three observations a day. The force of the wind does not change so gradually, that we could interpolate to conclude on observations in the interval.

If we wanted only the average force and direction, we could derive it in the long run from a smaller number of observations, only we could not employ astronomical, but local time, that we might learn the variation of the direction during the day according to the influence of land and sea-wind. Wenckebach in his paper: *Sur l'intensité et la direction du vent en Neerlande* <sup>(1)</sup> has fully proved this, though he could not dispose of hours conveniently selected. Naturally however it may be done more in details from twohourly observations, such as those taken at the lightship at den Hinder <sup>(2)</sup>. And besides we wish to have more than that, what is not to be derived from a few observations a day.

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(1) *Verhandelingen van het Koninklijke Instituut van Wetenschappen te Amsterdam Deel XII.*

(2) I have investigated the sea and landwind on our coasts before in my *Changements dependants du soleil et de la lune* 1847, corroborating the results of M. Wenckebach, and now by distributing the wiads of a day at the Noord Hinder in four series, according to the winddirection at 6 à clock in the morning having been NE—SE, SE—SSW, SW—WNW, or NW to NNE. For the two former categories the change of direction is nearly two points.



Especially, navigating nations long to study the prognostics of the wind. All are eager to know in what manner the storms proceed over the surface of the earth, where and when they begin to rise, and in what space of time they travel from one place to another, in different directions. For all this more than three observations are required. In the annals of the R. D. M. Inst. we therefore published in the first part of every volume the direction of the wind at that hour, at which the greatest force was observed. This is already something, since thus a strong wind then is always mentioned; but still it is not sufficient.

So it will be matter of discussion for the Congress to determine the minimum of windobservations that ought to be printed, and what must more over be communicated. Then a choice must be made between the signs for the direction. It appears that arches are the most worthy of being recommended, were it only for clearness and distinctness. The signs by letters are of different signification in diverse languages; the French mean West by O, the German Oost as the Dutch, the English give E for that direction; uniformity must be obtained, be it by letters, language or signs.

In addition to the direction, the force must be given either by a number attached to the sign of the direction, or separately. The latter has been done in the Dutch annuaries, first parts, for sparing room, perhaps less conveniently: because we should not separate two things, which only combined express the nature of the wind. If then we will state both direction and force, we may do so by printing the force



in two different ways, thick or thin, as we did in both the parts of the Dutch annuaries for denoting strong and weak winds; if we wish to distinguish more accurately the different degree of force, we may add, according to that degree, more or fewer feathers; not too many however, that the image may be not too diffused; thus in adding numbers, we should like not more than one cipher to be indicated according to the scale of Beaufort, where 10, 11, and 12 are very rare. It would not be bad, if we gave them arrows with the numbers 1—6, and for stronger windfalls arrows with the same numbers in large size. Especially those meteorologists, who wish to give a view of the state of the atmosphere, at many stations on one page, will try to combine both distinctness and conciseness. As to themselves, they may for their own country naturally select whatever mode they like, giving fully and explicitly the state of things.

Again the question will arise on the anemometer, the merits of each sort will be discussed. Those, which indicate the velocity of air, permit an easier and more accurate calculation of the mean movement of the air during a definite period of time, they integrate the products of velocity and time; on the contrary the anemometers, which give the amount of pressure, enable us to say not only: such was at that point of time the pressure, but moreover: that pressure came from such a point of the horizon. Their indications have in addition to their scientific importance a practical one, since for building hothouses and edifices covering a large surface, it is of interest to know, what may be the greatest pressure to be guarded against in



that direction, to which the surface is opposed. It appears from this, that we should prefer to have a measure of pressure to a measure of velocity. It is desirable that both pressure and velocity should be observed at some places, in order that we might ascertain better the relation between the two. We have the tables of Smeaton and the observations of Prof. Stamkart, who determined the velocity of smoke emitted from chimneys with the pressure, measured at the same time by an anemometer, or only taxed; but still special experiments are to be desired f. i. turning round a pressure plate with a given velocity and registering its indications.

I think it superfluous to inquire, whether the wind has a horizontal direction: so near the surface of the earth it must have. But in measuring the variety of clouds we have to attend to that question. Nor does the height of the vane matter very much, unless it be too elevated, ex. g. a 100 meters. In that case however I have often observed, that the wind direction indicated by the vane of the cathedral tower was different from that of a vane at twenty, or thirty meters. The former preceded, the latter followed the next day; in the higher regions another wind direction prevailed, that in the lower regions had not yet overcome the friction between the houses and objects on the ground. So an anemometer placed higher may give a prognostic; but the direction of the clouds affords a better one, since they are more elevated and quite free from such a friction.

#### § 11. *Observation of Clouds.*

Many observers have given their attention to



the height and direction of clouds. The latter may be found easily by placing ourselves behind the vertical edge of a wall, so that the cloud appears to ascend or descend vertically; the former requires more complicated observation and some calculation. Several methods have been proposed, but practical difficulties have occasioned, that this very interesting research has not been made so thoroughly as we should wish. An observer placed on a high tower surrounded on all sides with water, might conveniently apply the method given by Wartmann; but this is not generally the case. We venture to recommend the enlarged sextant of Dr. Krecke, or the aid of an assistant, that at the same time, at the two ends of a stick, two or three meters long, the direction may be determined in which a conspicuous point of a cloud is to be seen, or the telestereoscope of Helmholtz. If we know the angles  $a$  and  $b$ , formed by the direction of that point with the direction of the stick, whose length may be  $l$ , at the time  $t$ , and the angles  $a'$  and  $b'$  formed at the time  $t'$ , we may derive only by operating on some sines the height and direction of the clouds, provided we suppose that their height was on the mean while not changed; if we do not make that assumption, the calculation is some what longer.

In summer this observation will more frequently occur, since more different layers of clouds are then floating in different heights. The Congress will have to decide about all these points and others. How must these observations be booked in separate columns, or does it appear more appropriate to note



them at the bottom of the page, as they will not be filled up regularly, if not expressly for that purpose a captive balloon floating over the observatory be kept, provided with registering instruments, at the proposal of Sir Ch Wheatstone and recently of Mr. Jamin. The expense would be very heavy indeed, but the persuasion that all changes of weather come to a place from its surroundings, and that to these surroundings belongs also what is above, that therefore our utmost must be done to observe all phenomena in the upper air, — some may perhaps try to realize such a plan.

The quantity of clouds is not uniformly communicated. In taking it we regard at the same time their bulk and the transparency of the unclouded part of the sky. In two different ways the serenity is denoted. For our part we have hitherto estimated the serenity in ten degrees, after the example of our Nestor Quetelet, 10 denoting a perfectly clear sky. In Germany many meteorologists distinguish only four degrees and give the number four to an overcast sky, 0 to a serene one. Or it is feasible to appreciate ten degrees. Dr. Krecke in 1851 gave at Prague the same number to a sky, then being looked at, as M. Jelinek. Were it not best to reconcile the two methods in this way, that we estimated from 0 to 10, but used a higher number for a more clouded sky. I would readily adopt that mode, if uniformity were gained by it.

About the terminology for the form of clouds I do not think, that there will be much discussion. It may be that the objections of M Poey to the ancient



terminology of Howard be well founded. I venture to suppose, that no one will draw general conclusions from the observations of cloudforms, no more than was hitherto the case, and so the description and observation of them may be omitted and left to the local observer, to whom they may serve as prognostics. Only the cirrhi perhaps may be held in greater honour when they appear in a clear sky, showing that in the highest strata of the atmosphere iceneedles begin to be formed in great quantity, which descending will firstly give a halo and then snow or hail according to the season, in all cases a depression of temperature, if it was the warm current of air invaded by the polar current.

#### § 12. *Actinometer.*

We mentioned above our parabolic mirror to ascertain the cold of elevated regions. Its indication depends also on the transparency of the sky, but this latter circumstance is much better appreciated both by the cyanometer and the pyrheliometer, instruments not only of theoretical use, but which may be practically applied to insolation. Shall we give preference to Herschells actinometer, to that of M. Waterston recommended by P. Secchi, to Pouillet's pyrheliometer, to one of the ingenious methods proposed by Bunsen, or Roscoe? It is worth while comparing the indications of those different instruments, where the climate allows us to take a regular series of observations. In our climate, at such a low level, we have little reason to expect to get them. Even on days that we call



clear ones the air carries too much aqueous vapour, or if you like, nebulous globules and absorbs too much of the calorific rays. The cyanometer ought to be at hand to distinguish the faintest change in colour whereby the observations would not longer be comparable. I myself in the summer of 1847 made many observations with the pyrheliometer, but could not obtain such concurring results, as are communicated by Pouillet and Quetelet. When before ten o'clock the sky was of a dark blue colour, at ten, from eleven to twelve the colour became less and less saturated and a few little clouds began to form themselves.

We are more likely to proceed at more elevated places, on hills and mountains. A comparison of two series of observations simultaneously made at different heights would be very interesting. Therefore it is to be regretted that Prof. Piazzzi Smyth, when he sejournd on the Peak of Teneriffe had no opportunity of watching them, neither have the observers at the Theodulpass made them, which I strongly recommended at the Congress of Geneva.

In addition to what we already enumerated, various other highly interesting questions might be solved in this way, concerning the variations of heat radiated by moon and sun. Many have tried to draw conclusions from thermometrical observations on the periodical influence of the moon. I have discussed myself more of them than all others together, but from the first felt compelled to fix the attention on pyrheliometric observation at great elevations, as being highly preferable to observations of thermometers on the ground. Melloni has at the same time deduced that



influence from thermoelectric observation, and in the latter years Lord Rosse and Marié Davy have independently shown by them, that the full moon gives more heat than the new moon <sup>(1)</sup>; but of regular pyrheliometer-observations on mountain tops, in Switzerland, India or any region blessed by an everbright sky, nothing is heard. Moreover by the pyrheliometer the question might be settled, whether, as I supposed, heat is unequally distributed along the equator of the sun, or if there is a ring of nebulous matter revolving round the sun within the orbit of Mercury, unequally dense. If we find the former to be the case my period of 27 days, 682, synodic time, of revolution <sup>(2)</sup> might be accepted to ascertain by its aid, whether the protuberances or the sunspots are formed by preference in certain meridians. Why not? We have indications, that on the two sides of the equator of the sun, not the same number of spots arise, that different latitudes give a different number of them, that the quantity of heat according to P. Secchi, and also of chemical rays on the authority of Pr. Roscoë are different in different circles of latitude. To the latter I suggested the inquiry of a difference dependent on the longitude, and he promised to do so, but hitherto no disquisition on the subject has been published. Perhaps the duration of my period does not appear accurate enough, still it is between that of the period of sunspots at the equator and

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(1) Pogg Ann LXVII Phys d. Ch. und Changements de température dependants du soleil et de la lune. Utrecht Kemink fils 1847.

(2) Pogg Ann ib. Changements, Verslagen der Kon Mind van wetens. Verslag van de sectie vergadering van het Prov. Utr. Gen. 1868.



at higher latitude. Prof. Wolf has disposed the observations of sunspots according to this period and thinks they agree with it. But were it not so, I do not so much expect to see my period confirmed by the periodical variations of the number of sunspots as by the prevailing number formed in certain meridians. I hope have not dwelled too long on these observations, which I trust may lead to the solution of a great many important questions of Meteorology, Selenography, Heliography.

§ 13. *Aspect of the Sky. Atmospheric electricity.*

The aspect of the sky is not easily indicated by figures, yet simplicity in the representation is desirable. The Institute of the Netherlands early adopted the signs formerly employed by Barral in his „*Journal d'agriculture pratique*.“

Others signs distinct enough in themselves have been afterwards adopted by the editors of the „*Nouvelles meteorologiques*“ a journal, which by publishing side by side vast numbers of observations has done essential service. In England again other signs have been introduced. We should by no means forget the splendidly executed meteorography of Pr. Francis Galton. Which mode shall be universally adopted? I shall keep to the old ones, until a common system of representation has been agreed on.

We may here apply the same remark as with respect to the different scales and measure of barometer and thermometer: various methods lead to the same object; it is difficult to decide which is the



best; but great loss of time, labour, new liability to error will be occasioned, if the same method be not everywhere adopted.

I will not avow a preference for any, I will change any I have hitherto used, even that I consider most worthy of approval, provided it be universally adopted!

Finally our attention will be directed to the observation of atmospheric electricity. The most common method is that of Peltier, strongly recommended by M. Quetelet introduced at Brussels and Gent, and which for years has been followed at Helder and Utrecht, with this distinction, that, at the latter place, the electrometer remains stationary in a safe spot, and an isolated ball is raised above all surrounding objects, brought into momentary contact with the conductor, and then rapidly lowered, in order to transfer its electricity to the electrometer; M. Lamont has proposed a modification on this apparatus, which I have partly adopted; then there are those of Profr. Thomson, wonderfully simple for observation, and of M. Dellmann, both very sensible. Which instrument shall be employed? In every case, the observations must be reduced to a uniform measure, as is done at Brussels. Shall we confine ourselves to the three hours of observation, 8, 2 and 10 o' clock, or shall the selfregistering instrument introduced at Kew, be universally employed?

Ozone observations are no longer taken in such a general way, nor are they of so much avail, that the Congress will discuss them.



§ 13. *Observations at sea.*

A final question may be, whether observations at sea shall be a subject of discussion. Here we have to distinguish between meteorologic observations of air and sea, and between inquiries to facilitate and promote navigation. In as much as respects the former, we are indebted for many interesting investigations to different nations, Sweden, Denmark, France Portugal, Austria, relative to navigation, shortening of voyages. We all agree, that these may be safely left to more private interchange of opinion between the institutes in the Netherlands, in England, and at Hamburg, since other European nations do not appear to have paid very much attention to navigation. From America too important transactions may be anticipated, as they will likewise be published on as great a scale, as since last year has been done with the observations on the continent: yet the directions for the taking of observations call for some revision.

The Congress held at Brussels aimed at accomplishing too much, and thence many were discouraged.

Or how else shall we account for it, that especially in the Netherlands, which soon declined to observe these directions, most observations have been sent in?

In all probability it would have been sufficient to confine the observations to the end of the watch kept on board of a ship, and not to ask for too much information. If I may be allowed to state, what has in this country attracted most attention, and with respect to which we have been supplied with the most and the best observations, it will be this.



1. We require at the beginning of the logbook, a statement of the corrections of the instruments after they have been examined by persons appointed for that purpose by the central Observatory.

2. The number of days the ship was at sea.

3. The conjectured and observed position of the ship.

4. The heights of the barometer by the mercury barometer.

5. The direction and force of the wind.

For this purpose Mr. van der Trappen has proposed an instrument to facilitate the reduction of the true direction and force, from what they are not only estimated at, but measured, on board of the ship.

6. The temperature of the surface of the sea.

No one will deny to these observations their high practical and theoretical value <sup>(1)</sup>.

7. Deviations of the compass.

On entering a port, or whenever that is an opportunity at sea to veer the ship, the deviations are verified. The safety of the ship depends too much on the local deviations, that time may well be sacrificed.

It may be worth while to recommend Prof. Stark's Compass, having two needles placed one above the other, as the different directions indicated by these two needles show, whether it is necessary to verify the compass or not.

8. The temperature observations, if there be on board any place fit for taking them.

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(1) We refer not only to the Onderzoekingen met den zeethermometer by Capt. Andrau, to those of M. Cornelissen Director of the marine branch of the Institute, to the English Charts, but also to the temperature Charts or every square degree in the Atlantic, we are publishing now



Psychrometrical observations, how much rain fell to day, deepsoundings, may be safely left to the interest of captains themselves and to such opportunities as are afforded.

What is of most importance to navigation must have the greatest weight with us.

Where, as in the Netherlands, medals are offered to the log-books most accurately kept, they should not be awarded, unless the above directions have been strictly complied with.

Thus these subjects will probably be submitted to the judgement of the Congress, and also whether others shall be added, and if it be necessary to take observations at any other hours than such as were formerly in use among all seamen. We do not think so.

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May I by this paper have attracted attention, especially to some of the most important points, leaving undisturbed, whatever refers to more special inquiries. May it induce others likewise, to communicate their opinions, before the meeting of the Congress, on these and other points, which have escaped me!

And if thus prepared we deliberate at the Congress, may then every one be disposed to make trifling alterations in his measures, in his notations, in his methods, where he can thereby, without great sacrifice, contribute to uniformity, that the common aim may be easily attainable by every inquirer, and a view may be obtained of the simultaneous phenomena all over the globe.









*Principal publications of the R. D. M. F. to be had from the  
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Buys Ballot, C. H. D.,

Suggestions on a Uniform System of Meteorological Observations Royal Dutch  
Meteorological Institute

Utrecht 1872

Phys.sp. 119 qf

urn:nbn:de:bvb:12-bsb11018405-6